Contemporary economics and the crisis

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The fundamental failing of modern economics, or at least of its dominant mainstream project, is not that it was unable successfully to predict the recent crisis but that it is ill-equipped to illuminate much that happens in the economy at any time.

The latter is an assessment that I have advanced and defended on numerous occasions (e.g., Lawson, 1997, 2003). Contemporary mainstream economics relies almost exclusively on certain methods of mathematical deductivist modelling; indeed it insists that formalistic modelling is the proper way to do economics. My contention, defended elsewhere at length, is simply that these methods are in fact largely irrelevant to addressing social reality, and it is the insistence that such methods be everywhere utilised that accounts for the continuing sorry intellectual state of much of the modern discipline.

Recently, I advanced a framework of analysis that, I suggested, is generally relevant for social analysis, including understanding the nature of the recent 'crisis' (Lawson 2009a). In the course of developing the arguments of the paper containing that framework, I took the opportunity to critically reference a contribution by David Colander, Hans Föllmer, Armin Hass, Michael Goldberg, Katerina Juselius, Alan Kirman, Thomus Lux, and Brigitte Sloth (2008). I did so because the latter paper appeared to me to send the signal that the crisis teaches us that we need to develop different versions of the mathematical models than those hitherto used to guide policy. Although the Colander et al (2008) paper, as might be expected from such a collection of authors, is insightful, the noted response, I believe, is not the best one. Because the paper seemed to have been influential, not least in heterodox circles, I used it as a kind of foil to set out my alternative account. I was, and remain, particularly concerned that the very recent apparent rise in popularity of seemingly radical substantive theories, most especially those that are counted as Keynesian, should not be used merely to develop alternative mathematical models to those previously dominant.

If my arguments about the limitations of formalism are correct, it follows that the situation of modern economics represents a very significant misallocation of resources – almost all are given over to the mathematical modelling project. Yet the seriousness of this unhappy state of affairs seems still to go largely unappreciated. So when the editor of this journal, Edward Fullbrook, invited me to produce a short paper that covered some of the same ground as in Lawson (2009a), I was happy enough to comply. However the invite was rather unusual in its details. It proposed a debate of sorts between myself and Colander et al covering those particular aspects on which we appear to disagree. Further, this debate was to take the form not of a direct engagement but of each set of contributors marshalling or summarising arguments of our earlier papers to address the statement below formulated by Fullbrook himself. This then explains the orientation of what follows. The statement in question runs as follows.

> *It is agreed that the current economic crisis has shown that the standard models of academic economics are seriously wanting. Should the main emphasis of reform be on developing new formal models or to an opening up of economics to methods other than traditional modelling?*

I start by considering the evaluation of the current situation contained in the first sentence of Fullbrook's formulation.

> "It is agreed that the current economic crisis has shown that the standard models of academic economics are seriously wanting"

I assume that Edward Fullbrook uses the category of 'standard models' here just because, and in the same way that, it figures in the contribution of Colander et al (2008). These latter authors introduce the notion when writing: "The implicit view behind standard models is that markets and economies are inherently stable and they only temporarily get off track" (p. 2). In consequence, these authors argue, the standard models are incapable of successfully addressing the crisis.

Now whether or not certain specific models warrant being distinguished as 'standard' on the basis of their economic content, a feature of the situation of academic economics that is undeniable is that for a long time now the category 'modelling' has become synonymous with mathematical deductivist reasoning. The latter association, if questionable methodologically, is indeed a modern 'standard'. Thus any set of 'standard models' that Colander et al (2008) may identify according to the substantive content will be examples of mathematical deductive formulations.

My contention, explained and defended below, is that the fundamental problem of modern economics lies in its emphasis on formalistic modelling per se. So from this perspective, Fulbrook's evaluation above, or the assessment of Colander et al upon which Fullbrook seems to be drawing, is somewhat misleading, and in fact encourages an overly narrow focus and response.

I do not dispute the evaluation that the specific models that Colander et al (2008) designate standard 'are seriously wanting'; this is hardly contentious. The point, rather, is that just about all economic outcomes bearing on these (and any other sets of mathematical deductivist) models have indicated this for years, the economic crisis no more than anything else. And the reason (I will argue) is precisely the inappropriateness of the mathematical form of modelling per se as a general method of social analysis.

From this perspective, my concern is that by putting the emphasis on specific so-called standard (mathematical) models, the response (to the failure to illuminate the crisis) that is encouraged is that it is sufficient to put resources into developing alternative (less 'standard') mathematical-deductivist models (with the hope of accommodating [and perhaps even predicting] crises specifically).

Of course, the noted likely response of developing alternative formalistic models is only encouraged rather than necessitated by Fullbrook's and Colander et al's formulations. As the second part of Fullbrook's statement explicitly recognises, in practice the option of developing alternative approaches that do not take the form of mathematical modelling is not precluded. But with the culture and reward structure of modern economics so oriented to mathematical modelling, the formulation easily promotes precisely the noted reaction.

This indeed is the message imparted by the paper by Colander et al (2008). As I am supposed to be debating with these authors let me elaborate this assessment a little. There is a tendency in modern economic methodology (that I regard as unfortunate) which is to seek wherever possible to please all sides (or anyway to avoid upsetting any side) to any debate. The outcome, typically, is that either nothing or everything is supported, so that methodological advance is rarely made. I am not sure if being part of this tendency is the intention of Colander et al. Certainly, I note that they avoid stating explicitly or directly that any new approach is best formulated in terms of models interpreted as forms of mathematical deductivist reasoning. And although they certainly display support for modelling activities, I suppose that if pressed (especially if addressing a heterodox audience) they might suggest that they do not equate modelling to mathematical reasoning. But whatever the intention of Colander et al, or the interpretation they may prefer to put on their piece, I believe the signal they actually send, given the prevailing context, is support for yet more mathematical modelling. Let me indicate some of the reasons why I say this.

First, and most noticeably, having focussed on the failings of 'standard models', models that are inherently formalistic (the two assumptions Colander et al criticise specifically, namely rational expectations and the representative agents, are formulated precisely to render mathematical models tractable), Colander et al never raise the possibility that formalism in the academy (as opposed to the finance industry) may itself be the problem. For anyone at all aware of modern methodological discussion, this omission in itself is rather striking, and certainly telling.

Second, Colander et al adopt a prominent mainstream strategy and indeed mainstream language in ridiculing, rather than seriously engaging, alternative practices or 'remedies' to those employing 'standard' or other forms of models. As is well known, mathematical modellers tend to dismiss any contribution that is not formulated mathematically in derisory terms, such as 'hand waving'. This is the precisely the recourse of Colander et al:

> "Ironically, as the crisis has unfolded, economists have had no choice but to abandon their standard models and to produce hand-waving common-sense remedies. Common-sense advice, although useful, is a poor substitute for an underlying model that can provide much-needed guidance for developing policy and regulation" (p. 2)

The authors may or may not want to commit themselves on the meaning of the category 'model', but, as I say, expressing support for undefined 'underlying models' that are contrasted to hand waving in the context of modern academic economic discussion certainly encourages the reading that yet more mathematical deductivist reasoning is being advocated.

Third, the various specific constructive suggestions advanced are mostly (and most easily) interpretable as suggestions for revised formalistic models or formalistic modelling strategies and techniques. Thus, Colander et al suggest a possible need for "a different type of mathematics than that which is generally used now by many prominent economic models"(p. 3); that "considerable progress has been made by moving to more refined models with e.g., 'fat tailed' Levy processes as their driving factors" (p. 6); they argue for models that allow "one to study out-of-equilibrium dynamics and adaptive adjustments" adding that "Such dynamics could reveal the possibility of multiplicity and evolution of equilibria" (p. 9). They also conjecture, that "If one accepts that the dispersed economic activity of many economic agents could be described by statistical laws, one might even take stock of methods from statistical physics to model dynamic economic systems" (p. 10). In terms of method, they "recommend a more data-driven methodology " in which "data-analytical tools and specification tests" are employed, adding that "clustering techniques such as projection pursuit […]might provide alternatives for the identification of key relationships and the reduction of complexity on the way from empirical measurement to theoretical models" (p. 11); Furthermore, "Cointegrated VAR models could provide an avenue towards identification of robust structures within a set of data" (p. 11), adding that:

> A chain of specification tests and estimated statistical models for simultaneous systems would provide a benchmark for the subsequent development of tests of models based on economic behavior: significant and robust relations within a simultaneous system would provide empirical regularities that one would attempt to explain, while the quality of fit of the statistical benchmark would offer a confidence band for more ambitious models (p. 11)

And so on. I will not go on, not just because we may not have a dispute here (Colander et al may or may not resist my interpretation of them as ultimately contributing to sustaining the formalistic emphasis), but more significantly because, as I say, the primary concern of my earlier paper was not the contribution of Colander et al anyway, but the possible responses of heterodox economists.

However, whether or not their support for yet more formalism is a signal that Colander et al intended to send, I do regard it as a significant weakness of their paper that it fails to criticise explicitly the modern emphasis on formalism, or even to acknowledge the possibility that formalism per se may be the source of the failings of the modern discipline.

But to return to the main theme here, my worry was, and remains, that, as the crisis seems to allow a more significant voice than hitherto to heterodox lines of thinking, advocates of the latter may succumb to the temptation to focus on producing merely a revised set of mathematical deductivist models. Thus, for example, I worry that post Keynesians say may respond to the challenges before us by mostly advocating a different, supposedly Keynesian, form of mathematical-deductivist modelling.

Heterodoxy and mathematical modelling

The sense in which various traditions like post Keynesians are heterodox is precisely that they reject the mainstream or orthodox doctrine that methods of mathematical modelling should be used more or less always, and by all of us, whatever the context (I have defended this conception of heterodoxy at length elsewhere – for example Lawson, 2006). And heterodox economists have repeatedly rejected the particular models produced by the mainstream because they are recognised as being unrealistic in some significant way.

However, it is clear from a perusal of the range of heterodox writing and thinking that not all heterodox economists accept that the central problems of modern economics stem from the activity of mathematically modelling social phenomena per se. Nor is there uniformity within heterodoxy over the nature of the problem of unrealisticness in mainstream modelling.

Some heterodox economists seem to focus on the unrealisticness of mainstream conclusions; others on the wildly unrealistic nature of mainstream assumptions. Some economists in the former group seem to suppose that the realisticness or otherwise of assumptions is not really an issue; that even if a model is based on accepted-as-unrealistic assumptions, so long as it produces acceptable (for example, supposedly Keynesian) conclusions, then the whole analysis is satisfactory, and provides additional support for the preferred (because already accepted as true) conclusions. Economists in the second group seem to suppose that it is possible to construct mathematical deductive economic models that are cable of being explanatorily successful once, and whenever, the economic theorising is transformed in a manner that is more acceptable to the thinking of members of heterodox traditions.

The position adopted by the former group is, in truth, little more than crass opportunism. For if the constraint of employing only claims regarded as realistic is lifted, and I will argue that pursuance of the project of constructing mathematical-deductivist models of social phenomena more or less necessitates this, then for any preferred conclusion X, it is always a trivial matter to find a set of assumptions that facilitate a model consistent with X. I dealt with this issue in the earlier paper Lawson (1997a) and will not take space doing so again here. But it should be enough to point out that, so long as assumptions accepted-as-unrealistic are tolerated, it is not only trivially easy to choose assumptions that facilitate the construction of a model that is consistent with some preferred or desired or believed conclusion X, it is equally trivially easy to produce assumptions that facilitate the construction of a model consistent with the conclusion 'not X'. If a supporter of X thinks the procedure is somehow valid or useful in the first case, he or she must accept this is so in the second case too. Of course, the move is illegitimate in both cases. If we want to generate support for X it is necessary to do so on the basis of theorising and explanatory work that is not (regarded by everyone as) unrealistic.

So I turn, here, to consider the second possible response that concerns me, namely the allocating of available resources to yet more mathematical modelling activity, albeit of a sort that seeks to employ a different form (or set of theories) of economics. I do not suggest that such a response could never be the correct one (I return to this below). Certainly I do not suggest that post Keynesians or whoever should never try and develop insightful models (again I return to this below). But there are reasons to suspect that this response is unlikely very often to prove especially successful or useful. Let me briefly indicate what they are.

The problem with mathematical-deductive modelling of social phenomena.

My basic contention here is that with a bit of reflection both on the nature of social reality, and also on the sorts of conditions that must hold for the mathematical methods in question to have utility, we can not only better understand and explain the failings of the latter methods in the hands of modern mainstream economists, but also recognise that such methods are unlikely very often to provide insight no matter what substantive economic theories are used in their construction.

Simply put the sorts of mathematical deductivist methods in question are restricted in their applicability to closed systems, meaning those in which event regularities or correlations occur, whereas not only have such closures been found rarely to occur in the social realm, but also we have good reason to suppose they will remain uncommon.

In fact, closures are relatively uncommon even in the natural sciences. As it happens, outside astronomy, most of the event regularities known to natural science occur in conditions of controlled laboratory experimentation – or experimental closures. They arise when an experimenter succeeds in isolating/insulating an intrinsically stable mechanism from the effects of countervailing factors. Under such conditions a regularity can be produced correlating the triggering of the mechanism with its unimpeded effects.

Two conditions for guaranteeing a closure are apparent in this experimental case. The first is that we are dealing with a mechanism that is intrinsically constant. The second is that a situation can be engineered ensuring that this mechanism, if triggered, acts in relative isolation. We can refer to these as conditions as respectively the intrinsic and extrinsic closure conditions.

Although, other, perhaps very different, sets of sufficiency conditions are possible in principle, it is difficult to imagine what they might be in practice; and more to the point it is these two conditions – the intrinsic and extrinsic closure conditions - that mainstream economists mostly, if implicitly, seek to satisfy in their theorising around their economic models.

Of course whereas experimental natural scientists work laboriously to achieve the isolation of a relevant mechanism, economic modellers heroically assume that such isolations of intrinsically constant causal factors occur quite spontaneously in the social realm, and indeed are even ubiquitous.

However, it is easy enough to see that the phenomena of social reality by and large are such that the two conditions identified are unlikely very often to be satisfied.

Consider the extrinsic condition first. Instead of existing in isolation almost all social phenomena are in fact constituted in relation to each other. It is easy enough in modern capitalism to see the internal relationality of markets and money and firms and governments and households, etc; all depend on and presuppose each other. It would be futile and meaningless to seek to isolate any one from the influence of the others. But human individuals as social beings are likewise formed in relation to others. All slot into positions, where all positions are constituted in relation to other positions. Thus employer and employee presuppose each other, as do teacher and student, landlord/lady and tenant, parent and child, gendered man and woman, and so on. We all slot into, and are moulded through the occupancy of, a multitude of such positions, deriving real interests from them, and drawing upon whatever powers or rights and obligations are associated with those positions. So social reality is an interdependent, network, it is an internally related totality, not a set of phenomena each existing in relative isolation.

Nor does the hope of satisfying the intrinsic condition for a closure seem any more promising. For everything social (that set of phenomena whose existence depends on us) is constantly being transformed. Think of a language such as English. At any point in time it exists as a (largely unacknowledged) resource to be drawn upon in our speech acts and so forth. But through the sum total of all people simultaneously drawing on it, the language is (largely unintentionally) reproduced and in part transformed. It thus exists as a process, as something that is constantly being reproduced and transformed through practice. This is its mode of being; it is intrinsically dynamic and subject to transformation. But a moment's reflection reveals that all social phenomena share this mode of being: universities, towns, pollution, society at large, each and every organisation, our positions and their associated powers, our embodied personalities and everything else. So a satisfaction of the intrinsic condition for a closure again is something not to be taken for granted.

Of course social reality is more complex still. It contains meaning and value and so forth. But enough has been said to account for the general empirical failings of modern mainstream economics with its emphasis on mathematical modelling (as well as its employment of bizarre assumptions such as rational expectations, representative agents, two commodity worlds and all the rest that are maintained). This general failure is a result of the constant endeavour to present the phenomena of social reality that are really open, relational and processual as if instead they are closed, intrinsically constant and effectively isolated or insulated from each other.

So what is the response? This brings me to the second part of Fullbrook's formulation, to his question following the initial evaluation. It reads:

> Should the main emphasis of reform be on developing new formal models or to an opening up of economics to methods other than traditional modelling?

Irrespective of the arguments set out above, and no matter how successful or unsuccessful the project of mathematical deductivist modelling, the case can be made for 'an opening up of economics to methods other than traditional modelling'. The current dogmatic constraint on how we all can proceed is undesirable whatever the state of the discipline were found to be in terms of explanatory successes. If mathematical-deductivist methods were found to be useful at providing insight I suspect most of us would choose to use them. But it is vital to a healthy, intellectual, progressive enterprise that the ability so to choose does actually exist.

However, not only is such choice mostly absent, but so are the successes at providing insight. And I have suggested an explanation as to why. In the light of analysis set out, I not only support 'an opening up of economics to methods other than traditional modelling' but I believe too that this should be the 'main emphasis of reform'. Indeed, my central purpose here is precisely to caution against the usual response to failure which is to insist that the main emphasis of reform be on developing new formal models.

As it happens I am pessimistic that the sorts of mathematical-deductive methods that economists employ will ever provide much insight, for the reasons given above. However, let me emphasise that I do not thereby suggest that the development of new formal models and so forth be in any sense or form precluded. I support any situation where each individual is able to follow his or her own convictions in choosing which research path to follow. I am certainly not wishing to suggest that we replace one form of dogmatism by another. Although I am convinced by the analysis above, it could yet be found to quite wrong. And even if it is correct, the intrinsic and extrinsic conditions, as I have acknowledged, are only sufficient conditions for a closed system supporting an event regularity to emerge. A failure to satisfy them does not rule out the possibility of an emergent closure.

Indeed, an event regularity could even arise by accident. Though seemingly unlikely, it is impossible to rule out a priori a situation in which numerous complex, different and changing, observable and unobservable, transient and less transient, causal factors combine in such a way that, over a period of time and/or space, an event regularity is observed.

This seemingly unlikely outcome, in effect, is what macro economists hope for when they seek correlations in highly aggregated time series or cross-section data, where the different data points are produced in often quite different conditions and contexts. In such cases, the exercise (often systematised through theorising a representative agent) is based on little more than a heroic expression of faith, or maybe hope. Ultimately, just about all applied modelling endeavour utilises data that are aggregated to some extent and drawn from very different contexts, being produced by often very different causal mechanisms. Thus the recourse to little more than unreasoned faith and optimism is a pervasive characteristic of the modern discipline quite generally. Even so, my point here is that economists proceeding in this fashion may yet strike lucky. This cannot be ruled out in theory. So let the (mathematical) modellers keep trying.

But in the case of certain alternative approaches to such formalism, there is, in addition to faith and hope, both reason to expect explanatory successes especially where methods are tailored to conditions actually found to characterise the social realm, as well as evidence that such methods have already been fruitful (see Lawson, 2009b). So there is reason for the alternative approaches to be given some serious attention.

Institutional considerations

So far I have concentrated mostly on what might be termed the intellectual failing of modern economics, namely the misconception that utilising methods of mathematical modelling are a grounded, the best, and/or the only proper way of proceeding. But there is an additional, institutional, problem that explains why the failings of mathematical modelling have not led to a flourishing of alternative approaches, despite the demonstrated explanatory fruitfulness of some of the latter. This is simply that those with power allow almost no leeway for the undertaking of alternative approaches to formalistic modelling, despite the repeated failings of the latter, and indeed the demonstrated successes of alternatives (see e.g., Lawson 2009b or various contributions to Fullbrook, 2009). Those with power act as very restrictive gate keepers.

This is a very significant obstacle to intellectual advance. As already noted I do believe that individuals should have the real choice to proceed as they see best fit. And I have no desire to see experimentation with formalism formally excluded. But I also suspect that if the noted dogmatism were overcome, if this gate keeping were to end, the emphasis on formalism would likely change very quickly without any 'legislation'. It seems to me anyway that many economists use mathematical deductivist methods just because this is what is required of them, not because of any deep belief in their relevance or utility. As is widely recognised, it is mostly only modellers that get appointments in university economic faculties; it is mostly only such modellers that get promoted; it is mostly only modellers that get research grants from certain sources; it is mostly only PhDs and post doctorate research taking the form of mathematical deductive modelling that get funded; it is mostly only this sort of research that can get published in core journals, etc. (This is presumably the reason too that many methodologists mostly hold back from criticising the mathematical emphasis). Take away the insistence that only mathematical deductive modelling be supported and rewarded in the economics academy and I strongly suspect the composition of academic identities and practices will change very quickly, even if most of the current individual practitioners stay in place.

Of course, all of us should strive to maintain standards and seek to justify what we do. But this is precisely what the mainstream project currently fails to do. Mainstream modellers almost never justify the mathematical orientation of their endeavour, no matter what the extent of the failures of the latter. Nor is the modelling emphasis even questioned. When the results achieved are not successful, the response is almost always either to find a different set of questions to tackle, or to develop a different set of models, or modelling techniques, and so forth.

I might add that if some individual sincerely believes that there is good reason why experimenting with formalistic models is best not only for her or him, but for all of us too, that we all ought to be doing only mathematics, I am favour of their receiving a platform; I support their being heard and accommodated generally. They may even be right, though I currently strongly doubt it for the reasons set out. The problem is not the arguing for a methodological position but the current refusal of the mainstream modellers to engage in methodological debate (whilst simultaneously withholding opportunities and resources from those with different methodological convictions).

Of course, the noted intellectual failing and the institutional problem of modern economics are connected. The latter no doubt is a response to the former (as well as a cause of its continuance). If modern mainstream economics were widely successful in providing insight then I suspect its proponents would be more susceptible to interaction, debate, openness and tolerance of others. But at this point in time institutional power is about all the mainstream practitioners have in their favour. So it is perhaps not unintelligible, if a little disappointing, that they should choose to wield it in such a defensive manner.

I might finally stress that in arguing for a more intellectual forum in the economics academy, in suggesting that we replace methodological dogmatism with a more modest pluralistic orientation, I am proselytising not against rigour, but against the narrow supposition that it only takes one form. The position I defend does not even constitute an argument against the study of social phenomena being scientific in the sense of natural science. To the contrary, it grounds an argument that such study can be so scientific in the relevant sense, once alternative practices are facilitated; although that is another story (see e.g. Lawson, 1997, 2003).

Conclusion

The project of mathematical modelling in modern economics has a long history of failure. This is now widely acknowledged, even amongst mainstream economists (see Lawson, 2003, chapter 1). Less widely emphasised is a repeated pattern of response to this failure. It runs in two parts, involving first an evaluation, and then an inference. The evaluation is that ‘this specific set of mathematical models has performed badly because of that specific set of problems’. The standard inference is that the ‘solution comes with finding an alternative set of mathematical models that overcome that specific set of problems’. The proposals of Colander et al for dealing with the phenomenon of the recent crisis are easily interpreted as merely the latest version of this ‘solution’.

The concern I have with the evaluations and responses in question is that they detract from deeper ongoing problems. The first of these is that the sorts of practices of mathematical modelling that economists adopt seem continually to have failed to provide insight, and there are reasons to expect that things will not get better; that the methods themselves are inappropriate to social analysis. The second problem is that the economics academy is dominated by a mainstream group that posses, and utilises, a power to ensure that almost no approach except mathematical modelling is encouraged, published in core journals or otherwise rewarded.

My guess is that if an intellectual opening up of the academy can be achieved, an improvement in the relevance and utility of economics will quickly emerge as a matter of course. My concern is that so long as every failure is put down to limitations of a specific set of (formalistic) models, the explanatory weaknesses of the formalistic modelling process per se will continue to go relatively unchallenged, thus postponing yet further the day when the economics academy is transformed into the sort of open, honest and tolerant environment that seems essential for a generally successful economics to emerge.

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